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ABSTRACT

Over the past 30 years the study of politics has imitated the discourse of the natural sciences. Articles in this mode of discourse can be characterized as opening with a problem expressed through a review of the literature; continuing with an hypothesis, a methodology, and hypothesis testing; and ending with only narrow conclusions, limited to what can be documented by the mathematical argument. An examination of political science articles from the 1979 "American Political Science Review," however, reveals difficulties and complexities in this straightforward aspiration of political scientists to a scientific ideal. Arguments do not fit together as crisply as the ideal would have it, and the political scientists as authors inevitably find themselves in explanatory, justificatory, reconciliatory, and persuasive tasks that are not part of the idealized version of the scientific report. Problems arise in nomenclature, discussion of the literature, problem definition and significance, interpretation of data, authorial role, and relation to the audience. This rhetorical variety suggests that political science has yet to forge a consistent rhetoric. (Author/HOD)

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Getting the Damn Parts to Fit Together: Strategies in Writing a Science of Politics

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Getting the Damn Parts to Fit Together: Strategies in Writing a Science of Politics

Abstract

In recent years articles in political science have imitated the form of the research report in the natural sciences. Articles in this mode can be characterized as opening with a problem expressed through a review of the literature; continuing with an hypothesis, a methodology, and data testing the hypothesis; and ending with only narrow conclusions, limited to what can be documented by the mathematical argument. However, investigations of the discourse of natural science reveal that such a simplified format is an historical, social, and intellectual achievement forged in each discipline. Analysis of selected articles from the 1979 American Political Science Review reveals difficulties and complexities in the attempt to import the discourse of different disciplines; the authors inevitably find themselves engaged in explanatory, justificatory, reconciliatory, and persuasive tasks that are not part of the idealized scientific report. Problems arise in nomenclature, discussion of the literature, problem definition and significance, interpretation of data, authorial role, and relation to the audience.

In a narrow sense scientific texts are the primary product of pure science. If we set aside technological accomplishments and the craft-like skills developed in order to produce successful experiments, what science produces is knowledge--formulations, strings of alphabetic and numeric language (e.g., Latour and Woolgar, 1979, pp. 43-89). In Popper's terms science creates a third world of texts, standing indendepently of the nature they discuss and independently of the subjectivity of the texts' authors (Popper, 1979, pp. 109-152). The discourse of any science, therefore, should not be taken lightly, for a science can only know what its language can formulate, and what its language formulates is what it knows.

Over the past thirty years the study of politics has sought a new type of discourse, resembling the discourse of the natural sciences. The most obvious feature of this new discourse of political science is the reliance on mathematics for both evidence and argument. Most often the numerical data are gathered and analysed statistically, but sometimes the argument takes the form of abstract mathematical reasoning, as when game theory is employed. Articles assuming this mode of discourse may be more fully characterized as opening with a problem expressed through a review of literature that orders the existing knowledge in a coherent system of findings and issues. The article then proposes an hypothesis or solution to the problem, presents (and perhaps justifies) a methodology, then tests the hypothesis through mathematical data and argument. At the end only narrow

conclusions are formed, limited to what can be documented by the mathematical argument. In 1979 over 70 per cent (30 out of 42) of the articles in the American Political Science Review could be so characterized. The remaining articles, other than a presidential address statistically examining one aspect of quantitative political studies, are devoted to historical narratives about political movements and philosophical discussions of new and classical political theory. Moreover, the articles in the natural scientific mode averaged 1.63 authors per article compared to an average of 1.08 authors per article for the historical and philosophical texts. The multiple authorship implies a research team practice resembling that of the natural sciences, resulting from the complexities of data collection and analysis (e.g., Physics Survey Committee, 1973, p. 1368).

Such a textual organization is a direct correlate of the model of scientific activity presented in scope and methods books that explain to students of political science how to go about their intended profession (e.g., Greenstein and Polsby, 1975; Hayes and Hedlund, 1970; and Isaak, 1969). These books emphasize hypothesis testing and data collection as the core of science. Isaak, for example, discusses induction in the following terms (pp. 91-92):

> We test a hypothesis by seeing if it fits the world of observation. Suppose we want to test the

> hypothesis,"Businessmen tend to be conservative." A sample of businessmen would be questioned...to determine their ideological orientations. On the basis of this sample--and the confidence we place in our conclusions depends upon its size and randomness--we accept or reject the hypothesis.

The task of the political scientist is to compare claims to empirical reality; the function of political science writing is to communicate the findings of these comparisons.

A closer examination of political science articles, however, reveals difficulties and complexities in this straightforward aspiration to a scientific ideal. Arguments do not fit together as crisply as the ideal would have it, and the political scientist as author inevitably finds himself in explanatory, justificatory, reconciliatory, and persuasive tasks that are not part of the idealized version of the scientific report. This paper will explore problems raised by political scientific texts in order to suggest concerns that need to be addressed more fully and directly within the discourse of the field. First, however, we must look into the scientific paper as it actually exists in the natural sciences.

The first place most students encounter the idealized form of the scientific report is in high school science courses; there they learn to present problem, hypothesis, methods and apparatus, results and conclusions. The world of the high school

science laboratory is a very orderly and predictable place filled with well-defined objects, thoroughly established formulations from textbooks, fixed expert-amateur social relationships, and predetermined discoveries. The social, intellectual, natural, and creative worlds are held constant so that students can rehearse set operations to be reported in set formulations. Only by holding all these factors constant can language appear as an unproblematic direct representation of a sharply defined reality, a representation so transparent that it rises above the conditions of its utterance to achieve the Baconian ideal of unobscured knowledge of nature as it is.

Scientific discourse in actual research practice, however, is a complex set of tools serving in complex interactions. Each statement stands in relation to an observed reality, a body of prior statements, a contemporary situation among scientific peers, and the purposes, interests, and perceptions of the author. The scientific statement is a crystallization at one moment of a fluid set of circumstances (Bazerman, 1981a).

Nonetheless, the prose of some scientific specialties does approximate the appearance of the high school laboratory report and the Baconian ideal, but such prose is an historical achievement of disciplines that have forged simplified tools of communication based on well-defined, widely-shared, and unmistakeable referrents, relationships, and assumptions (Bazerman, 1981b). A primary element in this simplified communication is a precise and appropriate nomenclature through which the discipline defines the limited objects of interest, the sub-units of division, and even the conceptual style of the field. Focus of attention, manner of analysis, and even

theoretical possibilities are closely tied to the system of nomenclature. Elkana (1974), for example, argues that discovery of the law of the conservation of energy had to await the development of an appropriate term and definition for energy that sorted out the relevant phenomena along the right conceptual lines. Although nomenclature can always be changed and reconceptualized, a discipline's terminology provides a shared linguistic base against which changes can be negotiated.

The development of nomenclature--establishing a referential relationship between word and observed nature--is a consequence of the larger development of a codified literature, with some formulations generally accepted, others generally rejected, and with a shared understanding of the relationships among the many accepted formulations. This literature provides a base of theory and findings upon which new work may proceed. Even more, the literature provides a base of conceptual referrents (Small, 1978). Just as the nomenclature points to objects to be discussed, the literature as a whole provides ideas that may be used. Some ideas are so new or uncertain of status that they are the topics of discussion in other texts. Other ideas are so accepted that they are unthinkingly imbedded in th language, a process which Merton (1968, pp. 27-30) calls "obliteration by incorporation." Obliteration by incorporation makes the language of science highly and precisely idea-laden. In order to use the language properly, one must master all of the concepts imbedded in each term.

The theoretical base of the literature helps define the new problems to be addressed and the way in which the work is to proceed. Whether one defines the process of scientific

continuity in the restrictive terms of Kuhn's normal science (1962) or the more evolutionary terms of Lakatos' scientific programs (1978) it is clear that prior work establishes the open questions, the anomalies, and the promising lines of inquiry that set new work in motion and against which the new work is properly understood.

The codification of a literature depends on shared assumptions accepted in the field, among them assumptions concerning criteria of judgment: what constitutes compelling proof, what defines adequacy of empirical work, what are the inviolable principles of logic and nature than cannot be transgressed in an argument. Without a sharing of these judgments, explicit or implicit, there would be no basis for agreement over the literature. In order to make the joint endeavor of establishing and continuing a literature possible, there must also be compatable (if not shared) assumptions about the purpose, nature, and commitments of scientific activity. Whether one wishes to posit an ideal ethos of science as Merton does (1973, pp.267-278) or to observe a more fluid set of intentions and negotiations, the set of social actions and purposes must mesh in common activity.

This last, a common pursuit, implies some form of social organization with satisfactory means for mediating disagreements and misunderstandings. Again, whether one holds to the lock-step of Kuhn's normal science or Popper's more rational and open-minded critical operations, disputes and factions must have a means of being ameliorated so that the joint endeavor may continue with an adequately common view as

to how the work ought to proceed in both topic and method. Thus the author of each new contribution needs pitch the writing to a limited audience of limited characteristics; even more, the author need not appeal to the individual hearts and minds of individual readers, but need only appeal to an externally articulated set of shared goals, beliefs, and interests.

As I have reported elsewhere (Bazerman, 1981a), the famous paper by Watson and Crick (1953) announcing a structure for D N A, because of factors similar to those noted above, directly matched formulation against observed nature, so that the article appeared to be concerned only with saying true things about nature. Audience persuasion, criteria of judgment, evaluation of the literature, and authorial purposes were all subordinated to the claimed correspondence between proposed structure and actual molecule. The research problem, the objects discussed, the relation of the claim to existing knowledge, and the consequentiality of the claim were so tightly fixed by the structure of knowledge of the field that much of the wording of the article was unproblematic, if not predetermined. The historical-communal development of the field's language made the writing task--the putting together of words--easy for the two authors.

An investigation of the notes and rough drafts of the physicist Arthur H. Compton similarly revealed that so much of the final articles were determined by the development of the field and the history of the particular problem, that in the actual drafting and revision Compton was most concerned about the elaboration and separation of different aspects of the

data and full explanation of the phenomenon. In the drafts and revisions one finds mostly clarification, elaboration, and fine-tuning adjustments of representations of nature; the larger rhetorical elements were already fixed before Compton set down with blank paper (Bazerman, 1981b).

In one example of sociological writing, Robert Merton's "The Ambivalence of Scientists" (1973, pp. 383-412), on the other hand, a less stable situation is evident (Bazerman, 1981a). The meaning and consequence of large parts of the literature were more equivocal; the readers were divided by a variety of beliefs, interests and criteria of judgment; the problem was less self-evident; the persuasive task, more difficult; the methodology and argumentative procedure, more idiosyncratic; and the language, less technically univocal. The author had to contend actively with all these factors to make his case as effective and purposeful as it could be.

Such textual analyses suggest that the reporting of science inevitably occurs in social, intellectual, psychological, and natural contexts, which must be considered if not controlled. In older sciences much of these contexts may be simplified and accounted for by well developed language and linguistic practices, but in the newer sciences we need to attend to the kinds of adjustments being made to accomodate the particular contextual characteristics of the new field. An analysis of political science texts reveals that the wholesale adoption of an idealized version of the scientific report is not adequate to the needs of political science, as evidenced by the strains in the discourse and the ad hoc adjustments already being made by some authors.

The following observations are based on an examination of the American Political Science Review, volume 73, 1979 and a closer analysis of three articles in particular: Edward T. Jennings, Jr., "Competition, Constituencies, and Welfare Policies in American States," pp. 414-429; Diane L. Fowlkes, Jerry Perkins and Sue Tolleson Rinehart, "Gender Roles and Party Roles," pp. 772-780; Benjamin I. Page and Calvin C. Jones, "Reciprocal Effects of Policy Preferences, Party Loyalties and the Vote," pp. 1071-1089. I chose these three because they represent a range of topics and styles.

The most obvious difference between papers in political science and papers in physics or molecular biology is length. Watson and Crick's famous paper was about a thousand words long, comprising only one page of text; the papers of A. H. Compton on x-radiation (1973) are for the most part between a thousand and two thousand words in length. On the other hand, the mathematically developed papers in the 1979 APSR run from seven thousand to fifteen thousand words, tending toward the upper half of the range. The articles take from nine to twenty pages of closely packed, double columned pages. Apparently papers in political science cannot define a problem, propose a solution, describe a method, offer evidence, and draw conclusions nearly as compactly (and, therefore, one assumes, directly) as papers in the natural sciences. Interestingly, the other papers in the 1979 APSR, essays in political theory and historical politics, run about the same length as the mathematically developed essays. The old and the new discourse share a commonality of

expansiveness, if nothing more.

One thing political scientists spend many words on is discussing each other's work. The typical article in APSR has a bibliography of twenty to forty items--this is true of both mathematical and theoretical articles, although the theoretical articles tend to cluster around the upper numbers. The citation method in the journal actually tends to obscure the number of textual references; the three sample articles each contained from thirty to fifty references, discussions, or characterizations of other sources. ← This compares to six footnotes at the end of the Watson-Crick article and typically under ten sources used by Compton, although in a longer theoretical article he referred to sixteen sources a total of twenty-five times. In the three political science papers the references to sources were clustered at the beginnings of the articles in extended reviews of the literature(which in fact extended in one case half the article) and in the last few pages of conclusions.

The reviews of literature in the political science papers indicate a lack of codification. The reviews go back to assemble and discuss all the literature the authors feel bear on the subjects at hand. The article on welfare policies begins with V. O. Key's seminal comment (1949) and then discusses every major test of Key's hypothesis (1959, 1963, 1966, 1969, 1970, 1976); the article then returns to reinterpret Key's original comments. The author Jennings obviously cannot rely on the audience identifying and understanding the background literature in

the same way he does. Similarly, Page and Jones review thirty years of voter studies in nine pages and over seven thousand words; Fowlkes, Perkins, and Rinehart make mention of all of what they consider major works on women in party organizations and on differentiation of party membership, much of the work dating from the sixties. In contrast, sources cited in the physical sciences are likely to be quite recent because a shared understanding of the background literature and its significance can be assumed. Price, (cited in Merton, 1973, p. 507) for example, calculates that 72 per cent of the citations in the Physical Review are to papers published in the preceding five years. A similar calculation on the bibliographies of the three political science articles reveals that only 18 %, 47 %, and 47 % (or overall just under 30 %) of the sources are from the past five years. Moreover, in the reviews of literature in the natural science papers, references tend to be to the claims that either immediately precede the current claim or are direct competitors; as we have seen, in political science papers the reviews go back deeply to reconstruct and reinterpret whole problem areas.

This restructuring of the literature requires a broad stroke treatment of a large number of sources. Works are frequently categorized and treated as part of a group with only representative articles discussed in detail. In their seemingly detailed discussion of prior voting studies, as an extreme example, Page and Jones actually discuss their own versions of typical arguments and then list sources which they claim are similar in method. Brief general characterizations, group characterizations, and simple lists of sources are common in all

three articles. Such patterns of generalization rely on the audience's faith in the author for their persuasiveness; little compelling evidence can be given to justify the readings and judgments of the sources or to eliminate other equally plausible interpretations and evaluations. Selected detailed discussions of some sources do provide some specifics about the authors' readings of sources, but even here interpretations are rarely justified. For example, Jennings presents two summaries, each of about a hundred and fifty words, presenting "studies that can be interpreted to support [the preceding] analysis of the conditions under which characteristics of party systems might be expected to affect welfare politics." (p. 416) Although Jennings then summarizes the two sources in ways supporting his point, he never makes explicit the issues open to interpretation and the justification for his reading. The longer such an asserted interpretation continues, the more points of true question are likely. Compare this with the concise specificity of reference and discussion in the Watson-Crick article:

A structure for nucleic acid has already been proposed by Pauling and Corey. They kindly made their manuscript available to us in advance of publication. Their model consists of three intertwined chains, with the phosphates near the fibre axis, and the bases on the outside. In our opinion this structure is unsatisfactory for two reasons: (1) We believe that the material which gives the X-ray diagram is the salt, not the free acid. Without the acidic hydrogen atoms it is not clear what forces would hold the structure together, especially as the negatively charged phosphates near the axis will repel each other. (2) some of the van der Waals distances appear to be too small. (p. 737)

A well defined problem, clear nomenclature, shared knowledge, and shared criteria of judgment make the Pauling-Corey claim easy to identify and prone to sharp, directed criticism.

The ability to restructure the political science literature offers opportunities for presenting one's current work in the best, most significant light. With most prior work up for reinterpretation and criticism, one can emphasize how much of a new departure or solution to old gordian knots the ensuing paper will offer. All three political science articles I examined closely, in fact, claim all prior work misses the boat; the reviews of literature, consequently, become critiques of the fields in question. Fowlkes, Perkins, and Rinehart suggest that gender roles in politics have been incorrectly conceptualized in several ways. Jennings argues that "the logic underlying standard formulations of the interparty competition (IPC) hypothesis" (p. 415) is faulted and needs reformulation. Most totally, Page and Jones suggest "that virtually all past voting studies have erred by ignoring the possibility of reciprocal causal effects among the central variables of the electoral process." (p. 1071) The problem of each paper is simply to rectify the earlier mistakes. Watson and Crick, in contrast, do not have such luxury because the prior work defining the problem is so well established it does not require repetition. From the biochemical viewpoint everyone knows that D N A is an organic molecule residing in the cell nucleus, that organic molecules have defined structures, and that one current task of the field is to identify those structures, primarily through x-ray diffraction techniques. Moreover,

from the genetic perspective, everyone was aware of the search for a molecule that might serve as the carrier of genetic information. Both problems pre-existed Watson and Crick's formulation of an answer. The importance of the biochemical answer was that it provided a likely answer to the genetic problem, an answer so likely and so self-revealing that the authors felt little need to elaborate the genetic consequences. Watson and Crick only noted briefly, "It has not escaped our notice that the specific pairing we have postulated immediately suggests a possible copying mechanism for the genetic material." (p. 73 With all the parts in place, the authors needed only to fill in a final slot; they did not need to urge the importance of that act. They criticized only their immediate competitors whose work was so contemporaneous that it had not even appeared in print when Watson and Crick wrote their article. Even when Compton announced his discovery of what became known as the Compton Effect--a discovery which required a deep rejection of electrodynamic theory at the sub-atomic level--he still limited his criticism to a few specific articles that directly competed with his final claim. The problem and associated phenomena were too well established in the literature to necessitate a reconceptualization.

Both the Watson-Crick and the Compton discoveries set many new problems in motion; the consequentiality for related work is directly related to the degree of codification in the field. With the looser, more personally interpreted and evaluated literature of political science, any particular new finding, though interesting or striking, may not suggest

immediate followup work. Two of the three political science papers emphasized methodological innovations as well as empirical findings in order to establish the importance of the work. Thus the articles claimed to provide new ways of seeing as well as new things seen. A new way of seeing is clearly consequential for all further studies, even in fields where facts and hypotheses do not reverberate strongly off each other. In the longest of the three articles, on reciprocal effects in voting studies, the opening one-page summary review of the literature is followed by an eight page methodological critique of the literature; then three pages of description of the authors' new methodology follows. Under five pages are devoted to the actual presentation of data and discussion of findings. Of the ten paragraphs of conclusion, nine are devoted to methodological issues and only one to empirical discoveries. Of course Watson, Crick and Compton have made methodological innovations in the course of finding out what they wished to find out, but they discuss methodology in passing to establish the credibility (and perhaps reproducibility)of their work. In the natural sciences if a methodological innovation is considered worthy of major comment, the practice is to write a separate methodological article. New methodology is more likely to be viewed as enriching earlier limited views of nature, as when Compton employs Wilson's cloud chamber techniques to extend his earlier statistical results to measurement of individual x-ray-electron interactions. New methods are not usually considered as invalidating former findings unless a serious flaw is revealed in the earlier work, as when Duane challenged

Compton over the geometry of the box surrounding the target.

In political science the uncertainties of the meaning and consequence of data as well as the methodological conflicts that lead one to wonder what is the proper way to view any phenomenon seem to be at the root of another feature of the papers. The specialized statistical terms alternate with ordinary language discussions. The ordinary, common sense terms may be from traditional texts in political theory, from historical occurences, or from hypothetical situations. No matter the form, the specialized technical study seems not to be able to stand purely on its own terms. The studies seem to hang under the question, "What does all this have to do with the real world?" Yet this question does not seem to trouble studies in molecular biology or particle physics even though their data seem much further removed from the world of ordinary sense phenomena than voting statistics or per capita welfare expenditures. Jennings begins with common-sense paraphrase and quotation from V.O. Key and in the later statistical passages keeps converting the statistics into historical descriptions. Page and Jones rely on common sense observations about the 1972 and 1976 presidential elections to reinforce and interpret their data manipulations, and they let a series of plausible hypothetical comments carry an argument. And the gender role article steps back from a specialized statistical approach to a quite general set of speculations in ordinary language.

The lack of certainty about letting the technical argument carry the whole burden of communication may relate to another

problem--the uncertainty about what indicators are indicating and what the nomenclature refers to in the real world of political behavior. D N A is an isolatable substance, and its structure, as far as we know, is adequately portrayed in physical chemical terms. The terminology corresponds to our best view of nature. But in political science, although a vote is an isolatable measurable action, vote decision making is by its nature invisible to the outside observer; at the same time we have a wealth of anecdotal, testimonial, historical, and introspective data about the phenomenon. Any model we put forward is a speculation about an internal process that begs for comparison with our experience, knowledge, and intuition on the subject. No matter how detailed and concrete the data manipulations one can perform, one may suspect that the model does not really reflect the way things are. Even more of an indicator problem arises when you try to connect survey responses to actual behavior. The practical meaning of the terminology in the gender roles article was particularly befuddling. Suggestive psychological terms, although vague and not based on widely accepted theory, are made the basis of rather concrete distinctions. Thus, although the conclusions are intelligible in general terms, some of the specifics about how the conclusions refer to actual political behavior are elusive. Page and Jones address a similar issue directly when they try to identify the factors and relationships in their model of voter decision making:

> We cannot estimate any of the coefficients in Figure 6, as it stands, because the model is hopelessly underidentified. That is, there are only three empirically observable relationships among the central endogenous variables available to estimate the six causal processes of theoretical interest. (p. 1079)

The observable behaviors are not rich enough to tell them about the internal processes they are interested in. Page and Jones then try to define the internal machinery, but they run into further obstacles:

> It is in the search for suitable exogenous variables that difficulties mount, for most of the pertinent social theory is either not very powerful or not universally accepted. The grounds for specifying that a given variable theoretically cannot affect or be affected by another are seldom overwhelming. The situation is worse than usual when one deals with psychological measurements or attitudinal variables, since practically any attitude might conceivably affect any other. There are times when we seem to be studying relationships between mush and slush. [emphases added] (p.1080)

The authors escape from their dilemma only by an eclectic

synthesis of plausible factors suggested by the literature, history, and common sense. But no grounding theory or unifying approach make the factors and relationships anything more than assertions. To their credit, Page and Jones recognize their conjecture.

As well, this last case exemplifies the exposure of the author's intellectual processes, typifying an authorial role in political science papers that both resembles and differs from the authorial roles in natural science papers. Contrary to the stereotype, the first person is not expunged from the natural science papers, but its role is specific and limited. The first person expresses authorial actions and

manipulations--both physical and intellectual--as well as ownership and credit for the research process and results. For example, in an article by Compton and Simon we get such phrases as "the apparatus used in our work," "for illumination we used a mercury spark," and "our results therefore afford a strong confirmation." In the Watson and Crick article appear such phrases as "we wish to suggest a structure," "in our opinion this structure is unsatisfactory,""we have made the usual chemical assumptions," "we have assumed an angle of 36 degrees," and "it has not escaped our attention." Doing, thinking, and owning--I have found much the same uses of the first person in the political science articles. For example, from Page and Jones in addition to the previous passage are such phrases as, "we intend to specify and estimate," "we first consider," "we conceptualize," "we prefer to analyze," "we measured reactions to candidates' personalities by counting the net number," "in short we are suggesting," "we can with some confidence specify," "to us the most striking aspect," and "perhaps the theoretically most important of all our estimates." Similarly in the other two articles we find the authors represented as the doers, the interpreters, and the owners of the research.

Yet the political science articles differ from the natural science articles in the importance of that same authorial role. For all the reasons discussed earlier in this paper Watson and Crick or Compton can take the stance that, "I did the work, but the results stand on their own merit--out of my hands, but

in the framework of contemporary science. If you don't believe me, just go look at nature yourself using my methods or any other appropriate methods available to you." Thus the natural scientist, though using the first person, fades to the background. As Latour and Woolgar note, the claim seeks to rise above the conditions of its getting. The claims of political science may have the same ambitions of disembodied knowledge, but because of all the problematic conditions discussed earlier, the claims cannot easily rise above the author's perception of the literature, definition of problem, choice of methodology, naming and division of the phenomena investigated, and development of the argument. The author as conceiver, doer, and owner of the claim cannot so easily shift responsibility to nature for the truth of the claim. The authorial stance can be no more than "I have an interesting and revealing way of looking at political behavior and institutions. Look at them my way." Some readers come to share the vision and others do not.

In this way the discourse of political scientists still bears some resemblance to the discourse of political philosophers, who also ask the readers to see it their way, although the philosopher's vision is less constrained by empirical methodology. Rhetorically, political science is somewhere in the middle--whether that middle is part of an historical development or of a permanent dilemma I leave to epistemologists and future historians of knowledge. In the meantime political science needs the resources of both forms of discourse. The gender roles

article suffers from sidestepping its need for traditional discourse and using the stereotype of the scientific paper as a persuasive resource; we can see the rhetorical ambitions from the section divisions--untitled introduction, "Methodology,""Data Analysis,""Findings," "Discussion." The welfare policies article shows greater concern for the problem of translating the terms of ordinary political discourse into mathematically more solid form, as evidenced by the broader conceptual discussion preceding formulation of hypotheses and by interplay of historical descriptions and statistical indicators. Again the section headings reveal the stance; although the underlying structure of the paper follows the typical pattern of introductory review of the literature, hypothesis, methodology, data analysis, discussion, and conclusions, the division titles are more discursive: untitled introduction, "Party Competition and Welfare Policy," "State Welfare Policy and the Lower-Class Electorate," "Changes in Politics and Policy in Eight States," "System Differences and Policy Differences," "Electoral Support and Change in Policy," "Further Considerations," and "Conclusions." Thus Jennings preserves the appearance of a common sense political discussion even as he moves the argument into mathematical terms. Finally the study of voter decision making treats the scientific mathematical discourse it relies on as problematic. The underlying structure remains the typical one, but the review of literature is expanded into an extended theoretical methodological discussion. The division titles are then drawn from the

methodological problem: untitled introduction, "One-Way Causation: Recursive Models of Voting," "Two-Way Causation: Non-recursive Models of the Vote," and "Conclusions."

Each of the three political science articles discussed employs a strategy to maintain a stable rhetorical base on which to frame statements about real world political behavior and institutions. The articles share some points of strategy, but the overall stances toward the discourse differ. This rhetorical variety suggests that political science has yet to forge a consistent rhetoric. There are a number of ways to get the parts to fit together. But no matter how much you jiggle the damn parts, the fit is still loose. This rhetorician does not have a solution for you, but can only suggest that the more a discipline recognizes its condition in its way of talking to itself, the more precise and effective that talk is likely to be. If and when political science molds an appropriate and tightly fitting form of discourse, it will be an achievement won at the cost of some linguistic struggle.

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